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*presented by
W. O. Baldwin*

A REVIEW
OF THE
TRANSACTIONS

OF THE
MEDICAL ASSOCIATION OF THE STATE OF ALABAMA,

BY
J. B. GASTON, M. D., MONTGOMERY, ALA.

BARRETT & BROWN, PRINTERS.

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TRANSACTIONS OF THE MEDICAL ASSOCIATION OF THE STATE OF
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Although the epidemics of the past year—yellow fever, cholera and dengue—occupied much of the time of the Association, the variety of subjects discussed was quite sufficient to give spice and interest to the occasion.

The President's Address, by George A. Ketchum, M. D., on "The Sanitary Needs of the People of the State and the Related Obligations of the Medical Profession," teaches, in strong and sometimes eloquent language, that the obligation of the State to preserve the health of her citizens is quite as imperative as that to protect their lives and their property; that, in fact, the former duty is but a corollary of the latter.

This subject is of great national interest and statistics of death-rates largely diminished—fifty per cent. in some cases—by attention to the laws of hygiene, must impress upon thoughtful physicians the conclusion that more can be done in diminishing the ratio of mortality from disease by hygiene than by therapeutic agencies. The address is a valuable State paper, and we are pleased to see that the Association has printed one thousand extra copies as "a means of calling the attention of the people of the State and the attention of the General Assembly to the subject." State and municipal

* Extracted from Richmond and Louisville Medical Journal of December, 1864.

authorities should be taught that they, quite as much as the Medical Profession, hold the health and the lives of the people in their hands.

YELLOW FEVER.

The Transactions contain two papers on yellow fever—one by R. F. Michel, M. D., and the other by Jerome Cochran, M. D. Dr. Michel tells us—

- (1) That the fever was brought from Pensacola to Montgomery by *two persons*, and refers to his “History” of the epidemic, published in the Charleston Journal and Review, January, 1874, where he gives an account of the introduction of the disease and its propagation from person to person and from house to house, until it became epidemic.
- (2) That there were five hundred cases of the fever in Montgomery, with one hundred and eight deaths.
- (3) That a remarkable number of colored persons was attacked, but that few died.
- (4) That malarial and other fevers were rarely visible after the appearance of yellow fever, and cites authorities to prove that this has been usually the case in epidemics of this disease.
- (5) That there was no dengue in Montgomery in 1873; that the differences between dengue and yellow fever are “well marked and characteristic,” and that dengue is an exanthem, and “not haemorrhagic in character.”
- (6) That “the characteristic features” of black vomit were “first insisted upon by Dr. Middleton Michel in the Charleston Medical Journal, May, 1873;” and occupies four pages of the Transactions with an abstract of his paper.

Most of these points are very creditably sustained, but there are some exceptional features, and to these we will briefly allude. At the outset, Dr. Michel arrays himself under the banner of the non-contagionists, but in the same sentence, with *conspicuous impartiality*, tells us that he “believes as much in the local origin as in its being an exotic.” It is well that he thus early enrols himself as a “non-contagionist,” for in the absence of such declaration, his “History” might, with

great justice, be pleaded in bar of any such claim in the future. The "History" gives, we venture to say, as striking an illustration of the portability, the importation by persons, the propagation from person to person,—the contagiousness—of yellow fever as ever has been published.

Most physicians, we presume, would be very ready to admit that dengue is "not haemorrhagic in character"; yet we are sure all would not concur in some of the reasoning by which our essayist endeavors to establish this conclusion. "Pathologically considered, dengue," says he, "is one of the exanthemata, and being such, I do not believe it haemorrhagic in character." And again, in the conclusion of his argument on this subject, "I must therefore repeat that, pathologically considered, dengue is one of the exanthemata, and can not be regarded as haemorrhagic in character." Now, if the major proposition—no exanthem is haemorrhagic—could be established, the conclusion (if we admit Dr. Michel's view of the exanthematic nature of dengue) would follow necessarily. But since the Doctor mentions one haemorrhagic exanthem—"variola haemorrhagica"—and since others will occur to every intelligent physician, the untenable character of his major premise need not be further insisted upon. But not only is his major proposition false, but his minor—"dengue is an exanthem"—seems not to have the sanction of universal assent. Dr. Anderson says, "in some epidemics of the disease in Mobile the eruption has been the exception, not the rule." The usual course of argumentation is to proceed from the known or the admitted to a comprehension of the unknown or the doubtful. In the argument under consideration the rule seems to have been reversed. The premises, probably, more than the conclusion, need to be established.

A claim of priority of publication (which I construe the language of our author—already quoted—with reference to Dr. Middleton Michel's paper on black vomit to be) should, in justice to the medical public and to the party for whom the claim is made, be based so far as practicable upon the exact language of said party. This Dr. Michel has not given, or if he has (and the style and form of expression at times seem to

indicate that the investigator himself is speaking) he has used no marks to indicate where said language begins or where it ceases.

That this paper constituted at the time of its publication a valuable contribution to the literature of the subject can not be doubted by one who has observed the frequent and favorable allusion made to it by Dr. La Roche. That it contained a more detailed and systematic account of the characteristic features of black vomit than any previous publication I am inclined to believe. But the assertion of Dr. R. F. Michel, that "the characteristic features" of black vomit were "*first insisted upon* by Dr. Middleton Michel in the Charleston Medical Journal, May, 1853," requires, to say the least of it, a very thorough revision.

Dr. Henry Warren, 1740, insisted that black vomit was "mortified blood." Louis, 1828, insisted that "the black vomit is nothing but fluid blood, altered according as it has remained a longer or shorter time in contact with the mucous membrane of the stomach." La Roche, American Journal, 1854, while commenting on Dr. Middleton Michel's paper, and after stating that the microscope was first applied to the examination of black vomit in 1845, by John Davy, "with results similar to those obtained more recently," says that proofs of the sanguineous nature of black vomit "had long been regarded * * * by numerous and competent authorities as perfectly conclusive." Dr. J. C. Nott, American Journal, April, 1845, says that "black vomit is blood exhaled in its natural state from the capillaries of the stomach, intestines, and even the bladder, and changed black by the secretions with which it comes in contact; this chemical change, my facts go to show, is produced by one or more acids." "It invariably turned litmus paper red," and the filtered aqueous portion (which he describes in some cases as "limpid like water," in one as "light green," and in others as "deep brandy or rum color,") "in several cases effervesced strongly with carbonates." He attributes the "burning or scalding sensation in the stomach" to the "presence of acid," and "the varieties of black vomit" to "blood, acids, mucous and aqueous liquid, mixed in various proportions. Give me,"

says he, "blood, muriatic acid (or, I presume, any other acid) and gum-water, and I will make it to suit the notions of the most fastidious pathologists ; perfectly black, brown, reddish, etc." In all these *characteristics* Dr. M. Michel follows Dr. Nott pretty closely.

Dr. J. L. Riddell, New Orleans Journal, ix., 1852, says : "The black color is due beyond question,—[he *insists* upon it,]—to blood which, in all cases examined by me, bore the appearance of having been materially modified by acid. Most of the blood corpuscles seemed to have been disintegrated, broken down into small granules and irregular masses. Clots [microscopic] generally abounded, containing whole corpuscles, generally spherical, and smaller than the normal size ; from .00020 inch in diameter to .00030 ; the usual measurement being .00025." In one specimen "most of the corpuscles presented nearly their usual appearance, measuring from .00025 to .00033." Dr. R. made artificial black vomit by the addition of hydrochloric and other acids, and found "that the corpuscles resembled closely those usually found in black vomit." He discovered an abundance of epithelial cells, and gives their forms and the measurements of cells nuclei and granules. Other "vital organizations" are described, which Dr. R. thinks have no "special agency in producing or maintaining yellow fever."

From the foregoing extracts it appears that the description of black vomit published by Dr. Middleton Michel, May, 1853, had been anticipated, as regards every essentially distinctive quality of that liquid, by other writers, and that Dr. R. F. Michel is not sustained by the facts in asserting that "the characteristic features" of black vomit were "*first insisted upon* by Dr. Middleton Michel in the Charleston Medical Journal, May, 1853."

Although we have excepted to several propositions of Dr. R. F. Michel's paper, there is much in it that merits commendation. It has its excellencies, and to these, did space permit, it would give us pleasure to invite special and favorable consideration.

The Yellow Fever Epidemic of 1873, by Jerome Cochran, M. D.—Part first gives brief historical sketches of the epidemic

in New Orleans, Memphis, Shreveport, Pensacola, Montgomery, Calvert, Greenwood, and Mobile.

The number of cases and deaths which occurred in these places is given as follows :

	Cases.	Deaths.
New Orleans.....	2,000	226
Memphis.....	10,000	2,000
Shreveport.....	3,000	759
Pensacola.....	600	61
Montgomery.....	500	102
Calvert	450	125
Mobile.....	210	35
 Total	 16,760	 3,308

A review of the above-mentioned sketches teaches that the fever was brought to some of these places by vessels, to others by persons, and in two places—New Orleans and Shreveport—the origin is still involved in some obscurity.

It was brought to Pensacola by the Golden Dream, from Havana, which reached the harbor on the 10th of June, after having lost, at sea, eight of her crew by yellow fever. This vessel “was placed in quarantine, cleansed, fumigated and white-washed and detained twenty-four days before she was allowed to approach the city.” On the 2d of August, just forty-three days after the arrival of the Golden Dream—a period marked by no sickness on said vessel so far as we are informed—a sailor, eight days on board, was attacked with yellow fever, and “died with black vomit on the 5th.” “The first three cases that occurred in the city,” had no direct communication with the Golden Dream. They were, however, inmates of Mr. Orthong’s residence—just two squares from the water’s edge—and he had visited the vessel repeatedly. It was, therefore, not infected persons, but an infected vessel that brought the fever to the harbor of Pensacola. It was not in infected persons, but either in Mr. Orthong’s healthy person, in his clothing, or “as the more probable opinion seems to be, in the wings of the invisible swift winds that the seeds of the pestilence found agents of transportation” into the city,

The steam tow-boat Bee "reached Memphis on the 10th of August, and remained there several hours." Two deck passengers, sick with yellow fever, were put ashore in Happy Hollow and died the next day—one in Riley's shanty, Happy Hollow, and the other at the Adams-street station-house. From the latter place the disease did not spread. "The first victim claimed from the population of the city was a young man who had rendered some humane assistance to the poor stranger who died at Riley's. Riley himself was then stricken, and soon died." The disease was confined to Happy Hollow for "some weeks," and thence it spread throughout the city. From this account it seems probable that the fever was propagated in Memphis not from the tow-boat Bee, but from the anonymous stranger who died in Riley's shanty.

In Mobile, Montgomery, Calvert and Greenwood, the fever was introduced and propagated by persons who had come from localities where the disease was prevailing.

In connection with the progress of the fever in these several localities, the following circumstances seem to be especially noteworthy :

In Mobile the jail escaped altogether, and in Memphis "the jail, which was in the heart of the district that suffered so severely, escaped invasion until the 8th or 10th of October." These prisons are surrounded by high walls, and had little communication with the environs. In Memphis heavy rains had no perceptible effect on the fever. In Shreveport the fever became epidemic in *eighteen days* (?) after the appearance of the first case. The blacks were attacked almost as generally as the whites, but the mortality amongst them was very much less. On this point, Dr. E. D. Fenner, *History of Yellow Fever in New Orleans, 1853*, says: "During an epidemic he (the negro) will take the fever almost, if not fully, as readily as the white; but it will be altogether milder and less dangerous in its tendency."

Death from failure of the heart's action was observed in several instances; but there was no confirmation by post-mortem examinations of the views of Dr. Joseph Jones in reference to the fatty degeneration of that organ in such cases. Here let us observe that Dr. Nott, *American Journal, 1845*,

says, "the heart in some cases was found soft and flabby."

Dr. Faget's views with regard to the relation between temperature and heart-beat seem to have found some confirmation during this epidemic.

Part II. contains an admirable discussion of *the lessons of the epidemic*. In this part Dr. Cochran traces very clearly the revolutions and modifications of medical opinion during the last twenty years with regard to the propagation of yellow fever and its relation to our endemic paludal fevers. "Within the last twenty years," says he, "and very noticeably within the last ten years, the tendency of opinion on all these points has undergone considerable change. The doctrine of the affiliation between malarial fever and yellow fever has been completely overthrown, never any more to be revived. The doctrine of the transmission of yellow fever from continent to continent and from city to city by telluric or atmospheric waves, has been pushed into the back ground of speculation. And the doctrine of direct importation has been so thoroughly established by the occurrence of multitudes of facts as to admit no longer of controversy."

The doctrine that yellow fever is sometimes propagated from "germs that have remained over from some previous epidemic, germs that have been able to maintain their vitality from one season to another because of mild winters and other circumstances favorable to their preservation and development," "if true at all," says Dr. C., "is true only of one or two of our Gulf cities, and only occasionally even of them."

With regard to morbid poisons he cites the microscopic observations of Beale on the contagion of vaccinia; of Beale and Burden-Saunderson on the contagion of cattle plague; of Chaveau, of Lyons, on the contagia of small-pox, sheep-pox, cow-pox and farcy; of Coze and Feltz as to the nature of the several contagia of measles, scarlet fever, typhoid fever and septicæmia; of Klebs on pyæmia and septicæmia; all of which, with similar investigations by other observers, go to show a general agreement amongst authorities, that the septic agents of zymotic diseases are albuminous, colloidal,

solid and particulate. Some, with Klebs, believe that these particles are the spores of a special fungus. Others, with Beale, hold "that they are particles of living protoplasm of the diseased organisms; particles which have undergone organic degradation and functional perversion, according to the character of the pathological movement which seems to invest them with specific poisonous properties." Dr. C. admits that no one has seen the germs of yellow fever, yet affirms, upon analogical grounds, that such germs do exist and asserts that "there is no other theory capable of explaining the phenomena of the disease."

Both of the essayists on yellow fever, although advocates of the importation of this disease by persons and of its propagation from person to person, speak of it as infectious, and of its cause as an infection. One of them declares himself a "non-contagionist," and the other rejects the term, contagion, altogether in the discussion of this and kindred subjects.

In view of the controversy with regard to the mode of communication of yellow fever, carried on so bitterly a few years ago between the contagionists and non-contagionists or infectionists, we must consider this a somewhat questionable use of terms. The signification which the highest authorities on a subject attach to the terms used in their discussion of it ought to be conclusive as to the proper use and meaning of said terms. Etymology throws little light upon the *differentia* of such terms. As in pronunciation so in definition, the custom of the best writers and speakers determines the rule.

From such sources, speaking at a time when the origin of yellow fever in particular localities was very prominently before the Medical Profession, we learn that *contagion* was applied to a poison produced by disease in living beings and possessing the power of inducing like morbid action in other healthy living beings, either by contact, near approach, or through the medium of external bodies, such as fomites or

NOTE.—When these pages were written we had not seen Liebermeister's classification, in which "*contagious*," "*miasmatic*," and "*miasmatic contagious diseases*" are minor divisions of that class of diseases whose *poisons* "*can reproduce themselves, and to an unlimited extent*," which are termed "*infectious*."

the atmosphere impregnated with the poison; and that *infection* was applied to a poison not portable by the sick or convalescent, not communicable from person to person, and not produced as above mentioned, but originating in a concurrence of material and dynamic agencies external to human bodies. We do not hold that, at any time, there was a universal agreement in these distinctions, but we do claim that, at the time above mentioned, they were generally adopted by writers on yellow fever in this country. Nor does there seem to have been any change in the use and signification of the term *contagion*, for Niemeyer, October, 1870, says that "the word *contagion* is universally used in the sense that diseases which are transferred from sick to healthy persons are *contagious*." The distinction with reference to *infection* was never so generally recognized. It, however, has been recognized and adopted by some of the highest authorities of the last fifty years.

The impossibility of absolute quarantine, difficulties of local administration, and difficulties growing out of the imperfections of our scientific knowledge are admitted. Nevertheless, in view of the exotic origin and portability of the germs of yellow fever, the utility of quarantine is insisted upon. And since the general government regulates foreign commercial intercourse, its intervention is invoked to make and enforce such regulations as may be necessary to protect our coasts.

DISINFECTION.

The discussion of this subject is conducted with earnestness and ability; and a trenchant pen is directed against the protective virtues of carbolic acid disinfection as it has been practised in some of our Southern cities, especially New Orleans and Mobile. The utility of a certain restricted use of disinfection as a prophylaxis of yellow fever is admitted, but it is claimed that "all attempts to destroy the multitudinous germs of pestilence which, in epidemic seasons, are scattered broadcast through the illimitable kingdoms of the air must be abandoned as altogether hopeless." You can disinfect a

ship, but you cannot disinfect a city. You can disinfect the rooms of a house, but "to disinfect all *out-of-doors* is a problem of more embarrassment and difficulty than seems to be generally appreciated." The ineffectual disinfection of the Golden Dream, herein before-mentioned, illustrates not only the vital tenacity of the germs of yellow fever, but also the importance, nay, the absolute necessity of *thoroughness* in all attempts at disinfection in even the most circumscribed localities. "In view of the appalling mortality which has, on several occasions, occurred in connection with the use of carbolic acid" we are advised to be careful while attempting to suppress yellow fever with carbolic acid, that "we are not contributing something to the suppression of the lives of our fellow-citizens." An atmosphere so saturated with disinfectants as to assure the destruction of disease-germs, would, it is held, also prove fatal to human beings and domestic animals.

This paper, of sixty-two pages, concludes with a brief discussion of the nature of dengue and the characteristic differences between it and yellow fever, to which we will allude in another place.

Report on Dengue, by W. H. Anderson, M. D.—The facts and opinions of this paper are entitled to great respect, by reason of the large experience, the temperate and unbiassed judgment and the habitual accuracy of the author.

Attention is called to the "many different phases * * *" mentioned by intelligent authors who have described dengue in their particular localities," and our author says that the accounts of Dickson, Arnold, Campbell, and Fenner, "would not enable us in Mobile to recognize our dengue as the same disease which fell under their observation." Little importance is attached, as diagnostic symptoms, to the arthritis, the swelling of the joints, the decline of fever with a sweating stage on the third day, and its reappearance with insomnia, restlessness and vomiting, followed by an eruption, with relief of the above-mentioned distressing symptoms on the fifth or sixth day. These symptoms Dr. A. has "not witnessed; certainly not in the succession mentioned. In some epidemics

of the disease in Mobile, the eruption has been the exception, not the rule."

In the symptomatology of the disease the fact is mentioned that "there is little desire for cold drinks."

In the diagnosis, both Dr. Anderson and Dr. Cochran (the latter citing Dr. Ross as his authority) call attention to the desire for cold drinks in yellow fever as distinguishing that disease from dengue. In my limited experience with dengue, the selection of drinks has been very capricious. In one case nothing gave satisfaction until Congress water was tried. It was highly relished, and that, too, by a person not at all fond of it while in health.

The opinion expressed in the Transactions by different essayists with regard to the pathology of dengue, afford a signal illustration of the adage that *doctors will differ*. Dr. Michel, following Dickson, regards dengue as an exanthem. Dr. Cochran, with Faget, considers it a mucus fever, but differs with Dr. F. as to its paludal origin. Dr. Anderson holds that "the nervous system bears the brunt of the attack;" believes it to be "an affection of the ultimate filaments of the cerebro-spinal nerves after they enter the muscles." Attention is called to the fact that the extreme motor filaments may be paralyzed by a poison, while the trunk will not be affected by exposure to the same agent. The effects of woorara, when applied to the trunk and when applied to the ultimate filaments of a motor nerve, are mentioned in illustration of this proposition. So, it is held, the dengue-poison may produce morbid impressions upon the ultimate filaments, and be innocuous as regards the trunk of a nerve. The discussion of this branch of the subject and the interpretation of the various symptoms of dengue in accordance with this hypothesis are both interesting and ingenious. And although the Profession may not endorse Dr. Anderson's views with regard to the particular part involved, it will, we think, accept the general proposition that "the nervous system bears the brunt of the attack."

"As to its being allied in any way to *yellow fever*," Dr. A. "can see no shadow of evidence."

No novelties of treatment are proposed for the acute attack. Electricity, however, which is valueless during this stage, is recommended as an excellent remedy during convalescence. "No tonic, however, is as bracing as cold weather."

Cholera in Birmingham, 1873, by M. H. Jordan, M. D.—Mr. Stellar, the first sufferer and victim of cholera in Birmingham, received his "bed and bed-clothes" from Huntsville a few days before his attack, on the 2d of June, and "as cholera was prevailing at Huntsville at the time," (?) Dr. J. concludes that the infection was brought in this bedding to Birmingham. But when we are told by Dr. Dement of Huntsville, in the next paper, that the *first case* of cholera in that city occurred on the third of June—the very day of Mr. Stellar's death—the anomaly of an offspring older than its parent is at once presented and affords a sufficient excuse for our questioning the accuracy of the above-mentioned conclusion.

Dr. Jordan seems to be a careful and accurate observer of disease and his experience with cholera has developed the following interesting facts and observations:

"On the 5th day of July, Mrs. Hale had slight symptoms of diarrhoea, and concluded to go to her father-in-law's, at the top of Shades Mountain, about eight miles in the country. The next day she was attacked with cholera and died in twenty-four hours. Her mother-in-law, who waited upon her attentively and was constantly in the room with her until her death, took the disease in two days thereafter, and died in twelve hours." *The use of cistern-water afforded no protection whatever against the disease.* Dr. J. favors the moderate use of ice and ice-water by the sick, but condemns their free use as injurious and dangerous, the opinion of Reynolds, Aitken, and Yandell to the contrary notwithstanding. In the treatment of the cramps of cholera special attention is directed to the application of large dry cups along the spine from the occiput to the sacrum as never having failed to give relief.

Cholera in Huntsville, 1873, by J. J. Dement, M. D.—This paper gives a brief account of an epidemic in Huntsville, which, in a few weeks, carried off fifty-one of its inhabitants; seventeen white and thirty-four colored. The period of ineu-

bation in cholera, usually short, placed by Niemeyer at from thirty-six hours to three days and by others at from eight to twelve days, seems to have been extended in one case to fourteen and in another to twenty-three days. The only person attacked at Johnson's Wells, nine miles from Huntsville, was a gentleman who had left Huntsville two weeks previously. One person was attacked on Monte Sano, four miles from town, who had been absent from Huntsville twenty-three days.

The White Blood-Corpuscle in Health and disease; by Jerome Cochran, M. D., Mobile, Ala.

The doctrine of the genesis and structure of cells has, within the last twenty years, undergone great change and he who now speaks and writes of the nucleated cell, with its cell-wall and cell-contents, as the primary part in histology, must, in the light of current biological investigation, be regarded as a fossil of the Schleiden and Schwann period. It is to be regretted, we think, that the term, cell, has been so extended in signification as to embrace the homogeneous ameboid particle of living matter which is now regarded as the unit of organization. Authority, however, has so decreed it; and although its derivative meaning and common use suggest ideas and geometric forms which find no illustration in the white blood-corpuscle, we are taught that this corpuscle—without a cell-wall, non-nucleated, and non-differentiated—is a simple cell.

A brief captional analysis is all that space would permit in the way of a synopsis of this paper, and we will, therefore, simply notice some of its most salient points.

As a history and compend—comprehensive, yet compact—of current biological doctrines this paper is exceedingly valuable to the Profession and highly creditable to its author. The cell-doctrine is briefly but lucidly discussed. Scattered facts are arranged into a convenient system, so that they become endowed with an amount of physiological and pathological significance and importance which before has scarcely been suspected.

The most sweeping generalizations of biologists, however, are embraced and followed unhesitatingly and with unswerv-

ing fidelity to their logical extremes. Not only is the somewhat startling proposition, "that living matter is always and everywhere of the same identical nature; the same in its chemical constitution; the same in its physical properties; the same in its vital endowments," accepted as a "scientific demonstration"; but the identification of the white blood-corpusele and the amoeba—a legitimate deduction from the foregoing proposition, we believe—is adopted, and unqualifiedly affirmed. "Yea, verily, it is an amoeba," says the Doctor.

Now, is it legitimate to conclude because, with the various analytical appliances at our command, including the microscope, we can discover no difference in the living matter of plants and animals, that, therefore, they are absolutely identical; because, forsooth, they seem to be the same, that their identity is a scientific demonstration, to question which would be an evident absurdity? We think not. Do not the differentiations which follow close on this state of primal uniformity afford some ground to suspect that there may have been unobserved, transcendental differentiations in the living matter itself? Are there not good reasons for believing that the germs of a conferva and of an oak, of a zoophyte and of a man—all microscopically identical—are, after all, somehow or other, not exactly the same thing? Let us, hypothetically, transpose these factors of organization. Would the germ of an oak, resting upon the *membrana decidua*, so develop and so differentiate under the influence of the new circumstances and incident forces as to form a man? Would the fertilized human ovum placed in the *embryo-sac* of the plant, develop and grow, and spread its branches, a shade and resting place for the displaced and wandering plant-germ now developed into a man? Who can tell? That either of these results would happen is certainly not a scientific demonstration. That either of them would not happen is, we believe, the verdict alike of common sense and rational philosophy.

It may be claimed that the identity of living matter, always and everywhere, is a fact of observation which should not and can not be ignored until some heterogeneity is discovered. But it should also be remembered that the subsequent differ-

entiations of said matter are also facts of observation which can not be ignored. And in the explanation of these, it seems quite as philosophical to invoke the aid of transcendental differences in the primal, living matter itself, as to throw the whole responsibility on the incident forces.

Symmetrical diseases of the skin show that every part of the skin is different from every other part, except its symmetrical part, and there is no proof that even symmetrical parts are identical in structure. Symmetrical diseases of the bones illustrate the same fact with regard to the osseous system. And, although inappreciable to us by the most delicate chemical and microscopic analyses, there may be heterogeneity in the living matter of plants and animals. We can scarcely conceive of living matter more free from the disturbing influences of varying circumstances and forces than the segmentation-spheres of the ovum. Nevertheless, in them the most striking differentiations are inaugurated. "They march like soldiers to their appropriate places" in forming the blastodermic layers of the embryo; and this is but the beginning of a series of unparalleled differentiations observed during the development of the embryo and foetus.

Not only are we taught the universal identity of living matter, the identification of the white blood-corpuscule and the amoeba, the invariable community of character of the white blood-corpuscule with the lymph and pus-corpuscles, but the rigorous identification of the white blood-corpuscule with the germinal vesicle and the segmentation-spheres of the developing ovum is a speculation upon which Dr. Cochran especially insists. In view of these conclusions, he may well exclaim: "The stone so long rejected of the builders has become, indeed, the head of the corner; the foundation stone upon which must be constructed the whole edifice of the physiology of the future."

The division of one bioplast into two bioplasts—one cell into two cells—solves, it is held, the whole mystery of reproduction. Sex is not a primary and indispensable factor in reproduction; but the new creature, in the first stages of its development, is a bud springing from the body of the mother,

while the office of the male element is to supplement the development of the female factor, so as to raise the creature, so begun, to a higher development than it would otherwise be able to reach.

While the division of one bioplast into two bioplasts is the essential feature of reproduction, the fusion of two bioplasts into one bioplast is the essential feature of sexual conjugation. This latter act restores to the new creature the waning power of growth and development, "breaks up, in the most thorough manner, the paralysis of equilibrium which is stealing over them both, and in the complex mass, which results from this union, sets all the wheels of life into active motion."

It must not be supposed that we have attempted a synopsis of this paper. This, in the beginning, was specifically disclaimed. Many of its subdivisions have not been mentioned. Indeed, we have mentioned only those speculations which seemed to be impressed, to some extent, with the character of the author's own thought; and we take leave of the subject, regretting that the readers of these pages can not experience the pleasure which we have received in reading and studying the original paper.

Hemorrhagic Malaria Fever. By E. D. McDaniel, A. M., M. D., Camden, Ala.

To name a disease is one thing; to define a disease is quite another thing. A definition should embrace the essence of the thing, or such an idea of it that, being admitted, all the phenomena follow by implication. Some names—just as good as any too—are entirely arbitrary. Others are, to a greater or less extent, definitive or descriptive. To this latter class belong most of the names which have been applied to the disease under consideration. Of these Dr. McDaniel mentions nearly a score, and as the result of their somewhat rigid analysis, proposes "*intercyclic hemorrhagic malarial fever*," which he claims is not only "a significant and appropriate name," but "a perspicuous, precise and differential definition." As a name it certainly does comprehend more of the characteristic features of the disease than any which has been

proposed, but on account of its length, is, we think, somewhat objectionable. As a definition, it is, on the contrary, very compact, and presents both the *genus* and *differentia specifca* of the disease, so far as established by the literature of the subject and is, therefore, unexceptionable.

We consider this a good paper, and especially valuable on account of its discussion of the therapeutic value of quinine in this disease. In view of the malarial origin, frequently remittent or paroxysmal type of the disease, and the well known properties of quinine, no physician, we presume, would in his first experience with its treatment withhold this most efficient anti-malarial and anti-periodic agent. At one time all relied on it, and it still holds the unwavering confidence of many of the first physicians of this State. Some use it very heroically—use it “internally, externally and eternally.” But what has been the result of the quinine-treatment? A fearful mortality, averaging probably twenty per centum. Such results coupled, in some instances of distinctly intermittent or remittent type, with the signal failure of quinine to arrest the paroxysms and an aggravation of the haematuria under its use, have caused some physicians, including Dr. McDaniel, to regard quinine with great suspicion, and others to abandon it altogether; and the results have sustained them in their course. Of this latter class, some administer tinct. fer. chlo. very freely—from a half teaspoonful to a teaspoonful every two or four hours—until the haematuria and paroxysms are arrested. Others claim a good success, who do not use this remedy, or, if they use it consider it of secondary importance in the management of the disease. “In conversation on this subject with physicians and others,” says Dr. McDaniel, “I have had my attention called to cases in which haematuria followed quinine apparently as effect does cause. And over and over, and over again, have I in my own practice observed the haematuria re-established or aggravated after quinine; and, on the other hand, have seen the disease, with a very formidable array of symptoms, go handsomely on without one grain of quinine to a happy convalescence.”

The experience of Dr. East, Galveston Medical Journal, is quoted as entirely consistent with and confirmatory of these views. "As often as he got his patients into a satisfactory condition, and then commenced giving quinine, just so often he *plunged them back into haematuria*, with its unnumbered woes and dangers."

Dr. A. T. Pearsall, of this city, the originator, so far as I am informed, of the heroic iron-treatment, writes me that he has treated fifteen cases of haemorrhagic malarial fever since he adopted the treatment by tincture of iron, and that all of the cases so treated have recovered. Dr. McR., of Lowndes county, equally fortunate in the management of the disease, uses tincture of iron, but not so heroically, and with less faith in its curative powers. He attributes his success quite as much and more, indeed, to the abandonment of quinine and to other agencies to which he, in common with most other physicians, usually resorts, than to the use of iron.

From the facts and opinions found in Dr. McDaniel's paper, from the results of our own very limited experience and the comparatively large experience of other physicians—brought to our attention by letter and otherwise—in the treatment of this disease both with and without quinine, we are led to the conclusion that this agent is in very many, probably in most, instances not necessary, and in some positively injurious in the management of this form of malarial fever.

Puerperal Eclampsia. By F. M. Peterson, M. D., Greensboro, Ala.

This is a well written and, in the main, an excellent discussion of the subject. Albuminuria is in itself a condition of little importance, but as a sign of "a pathological condition which prevents the excretion of the poisonous principles which result from *tissue-metamorphosis*," it becomes invested with peculiar significance and importance. That albuminuria is not, however, an invariable attendant upon uræmic intoxication, is illustrated by the case of eclampsia reported by Dr. Peterson. The immediate cause of this affection of the nerv-

ous system—this neurosis of motility, as Braun calls it, is, as is well known, a toxæmia. The intoxication is uræmic, and results from a diseased condition of the kidney. The identity of this condition with Brightian degeneration, which has been affirmed by high authorities, is, upon plausible and apparently sufficient grounds, denied by our author.

Without restricting the intoxication to any one of the constituents of urine, our author points out possible transformations of urea and uric acid into several isomeric poisonous compounds, any one of which, in the blood, would be destructive of health. Frerich's doctrine, that carbonate of ammonia resulting from transformation of urea under the influence of a ferment, is the toxic agent in uræmia, is very summarily disposed of by the assertion that "the conversion of urea into carbonate of ammonia is the most favorable chemical change which it can undergo, as in this form it can be more readily excreted, and is less harmful in the organism."

Dr. Peterson certainly has not seen an account of Dr. W. A. Hammond's experiments, published some fifteen years ago; for no one at all familiar with the results of said experiments could, for a moment, maintain the innocuousness of any considerable quantity of carbonate of ammonia in the blood. The presence of a ferment in the blood being admitted, the transformation of urea into carbonate of ammonia might occur; and having occurred, the explosive violence of puerperal eclampsia would not be an improbable result. But the presence of a ferment is a mere hypothesis, and the occurrence of any such transformation has been rendered extremely improbable, if not absolutely disproved, by the experiments above referred to, published in *A. Med. Chir. Review*, March, 1858. An analysis of these experiments shows:

1. That while both urea and carbonate of ammonia injected into the blood vessels of sound animals produce decided symptoms of poisoning, neither causes death, the animals being relieved by the excretory action of the kidneys.
2. That both urea and carbonate of ammonia cause death when injected into the blood vessels of animals whose kidneys have been extirpated.

3. That in neither condition does urea undergo transformation into carbonate of ammonia.

"I desire to express the opinion," says Dr. Peterson,
* * * "that the retention in the blood of an excess of the poisonous principles of the urine from any cause constitutes the dominant factor in the etiology of eclampsia gravidarium, parturientium et puerperarium."

In another place the Doctor says: "A large majority of the cases of eclampsia puerperalis * * * are due to retention in the blood of the products of dissimilation, and their poisonous effects on the nervous system."

While these views are orthodox the fact seems to be that none of the exclusive theories of intoxication in eclampsia have been established. Whether the intoxication is due to urea, uric acid, or to some other constituents of the urine, or to their isomeric poisonous derivatives; or whether it is due to none of these, but to some of those primary products of tissue-metamorphosis—creatinine, creatinine, and other extractives—which are finally converted into urea and uric acid, has not been demonstrably determined.

The whole question with reference to the specific factor of intoxication in puerperal eclampsia must, therefore, be regarded as still *sub judice*.

Unavoidable circumstances prevent a notice of the remaining papers. This is regretted, for some of the essays omitted in this review are amongst the most meritorious contributions to this highly creditable volume of Transactions of the Medical Association of the State of Alabama.

